

LETTERS TO THE EDITOR.

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Science at Oxford and Cambridge.

I HAVE read with great interest Prof. Perry's article on "Oxford and Science" and his letter in NATURE of January 21, and assuming as I do that his remarks apply equally to Cambridge, I know that he has in no way overstated his case. There are one or two effects of the present system which I feel that he has scarcely brought out sufficiently strongly, and on which I lay the more stress, as I consider that they are harmful in more ways than one.

I well know the asphyxiating atmosphere of which he speaks, and I compare it to that of a septic tank the contents of which are reduced in time to the form of an innocuous but useless effluent. Had they been spread out over the country at large they would have been of value in raising up a fertile growth of scientific progress.

The university professor is generally in a position to disregard the apathy of his university, and to pursue researches for their own sake. In the case of the enthusiastic student who is desirous of embarking on a career of teaching combined with research, the effects of the present system are far more deadly, especially if he belongs to a small college where the mediæval atmosphere is usually most concentrated.

In the present day it is generally impossible, and if possible it is always highly inexpedient, that a man should devote his energies to research, pure and simple, without taking some part in the educational work which is being carried on all around him. But the man who, after taking a brilliant degree in arts or science, seeks to associate himself with the teaching and examining work of his college or university, frequently finds himself balked at every step by the opposition of a hostile but influential clique, although he is being continually urged by his friends to remain at the university in the hope that he may ultimately obtain that recognition which is freely conferred on men of less originality. Although the dons of his college will not raise a finger to help him, they do everything in their power to dissuade him from engaging in such outside work as a man without teaching experience has a reasonable prospect of obtaining in these days of competition. When a good professorship falls vacant, they write testimonials belauding his original work, of which they know nothing, but his candidature breaks down as soon as questions are asked about his teaching experience.

[A striking contrast to this spirit is seen in the excellent work done by certain well organised departments, such as the Cavendish Laboratory and a few enlightened colleges.]

What I have attempted to describe is not the experience of a single individual; from the number of cases that have come before my notice I feel sure that it must be a common experience.

I now pass to the other side of the question. When a vacancy occurs in a university college, it frequently happens that there is one candidate whose brilliant distinctions place him far above his rivals, and whose appointment would in all probability greatly conduce to the success and welfare of the department of which he would have charge. The electors would gladly appoint him if any definite evidence could be adduced as to his capability of discharging the duties required of him, but failing such evidence they are obliged, after a long and protracted discussion, to choose the second candidate on their list.

I know men who have broken through the barrier, both from Oxford and from Cambridge. I am glad to have such men as colleagues, for I know that they are doing splendid work in raising up a high standard of university education throughout Greater Britain.

Our university colleges have not been afraid to establish scholar assistantships in departments in which the work is too heavy for the existing staff. Why should not the same procedure be adopted at Oxford and Cambridge? This would often enable the colleges to give their best graduates

a good send-off into the world, and it would relieve the present teaching staffs of much burdensome routine work. We might even have college tutors waxing enthusiastic over scientific research! G. H. BRYAN.

The Radiation from an Electron moving in an Elliptic, or any other Orbit.

I HAVE been looking for a tolerably simple way of expressing the radiation at a distance from an electron, to avoid the work involved in reducing the general formulæ (NATURE, November 6, 13, 1902) in special cases. The result is

$$\mathbf{E} = \frac{\mu Q}{4\pi r} \ddot{\mathbf{s}} \sin \gamma, \quad (1)$$

subject to

$$R = v(t - t_1). \quad (2)$$

Here understand that Q is the charge moving in the path defined by the vector \mathbf{s} from the origin at the moment t_1 , and \mathbf{E} is the electric force at the corresponding moment t at the point P at the end of the vector \mathbf{r} from the origin, at distance R from Q , and γ is the angle between \mathbf{r} and $\ddot{\mathbf{s}}$. That is, the electric force is the tangential part of the vector $\ddot{\mathbf{s}}\mu Q/4\pi r$, or the part perpendicular to \mathbf{r} . The magnetic force is perpendicular to \mathbf{E} , given by $\mathbf{E} = \mu v \mathbf{H}$. It is assumed that s/R is very small, but no assumption has been made about u/v , so the waves are fully dopplerised. The dot indicates time-differentiation at P .

Example. Elliptic orbit. Let

$$\mathbf{s} = \frac{1}{n} \left(iu_2 \cos nt_1 + ju_1 \sin nt_1 \right) \quad (3)$$

Then Q describes an ellipse in the plane x, y , axes u_2/n and u_1/n , where $n/2\pi$ is the frequency. It is the spring or pendulum kind of elliptic motion. Describe a spherical surface with centre at the centre of the ellipse, and project \mathbf{s} upon the surface, and insert the result in (1). Then we get

$$E_\theta = \frac{\mu Q}{4\pi r n} \cos \theta \frac{d^2}{dt^2} \left(u_2 \cos \phi \cos nt_1 + u_1 \sin \phi \sin nt_1 \right), \quad (4)$$

$$E_\phi = \frac{\mu Q}{4\pi r n} \frac{d^2}{dt^2} \left(u_2 \sin \phi \cos nt_1 - u_1 \cos \phi \sin nt_1 \right), \quad (5)$$

expressing the θ and ϕ components of \mathbf{E} at the point r, θ, ϕ , if θ is measured from the z axis, and ϕ from the plane z, x .

Yet one thing more. The connection between t and t_1 is

$$nt_1 = n \left(t - \frac{r}{v} \right) + \frac{\sin \theta}{v} \left(u_2 \cos \phi \cos nt_1 + u_1 \sin \phi \sin nt_1 \right), \quad (6)$$

which gives

$$\dot{t}_1 = \left\{ 1 - \frac{\sin \theta}{v} \left(u_1 \sin \phi \cos nt_1 - u_2 \cos \phi \sin nt_1 \right) \right\}^{-1}, \quad (7)$$

which is required when (4) (5) are differentiated. This process introduces the factor \dot{t}_1^3 , and so, at high speeds, converts the radiation into periodic pulses, as in the case of a circular orbit (NATURE, January 28, p. 293). Put $u_1 = u_2 = u$ in the present formulæ to reduce to the circular. The analysis to simply periodic vibrations may be done in a similar way. If the motion in the elliptic orbit is of the planetary kind, the equation (3) is replaced by a much less manageable one. Electrons can conceivably vibrate in both these ways, according as the centre of force is condensed positive electricity, or is the centre of diffused positive electricity.

This is not the place for detailed proofs, but I can indicate one way of representing the matter which has some interest apart from the speciality of orbital motion. Given that Q is moving anyhow, it may be shown that my general formula for \mathbf{E} may be converted to

$$\mathbf{E} = \frac{\mu Q}{4\pi} \ddot{\mathbf{R}}_1 + \frac{\mu Q v}{4\pi R^2} \left(\dot{\mathbf{R}} - 3\mathbf{R}\mathbf{R}_1 + v\mathbf{R}_1 \right) \quad (8)$$

This gives \mathbf{E} at P , at distance R from Q , and \mathbf{R}_1 is the unit vector \mathbf{R}/R . The centre varies as we shift P , because Q is moving. It is always to be understood

that Q and P are at every moment of time uniquely connected when $u < v$. Any value given to t fixes a corresponding value t_1 for Q , and its position as well. This formula (8) is a very curious way of representing \mathbf{E} , and physically very unnatural. But the form of the first part is such that it leads easily to the radiational formula above given. Reject the second part of \mathbf{E} in (8), because it varies as R^{-2} . Then carry out d^2/dt^2 , and reject the R^{-2} part again. There is left

$$\mathbf{E} = \frac{\mu Q}{4\pi R} (\ddot{\mathbf{R}} - \ddot{\mathbf{R}}_1) \quad (9)$$

Lastly, put $\mathbf{R} = \mathbf{r} - \mathbf{s}$; then $\ddot{\mathbf{R}} = -\ddot{\mathbf{s}}$; and if s/r is very small, $\ddot{\mathbf{R}} = -\ddot{\mathbf{s}}_1$. So we come to the formula (1) above, as required. I hope this will be satisfactory. If not, there are lots of other much more complicated ways of doing the work.

OLIVER HEAVISIDE.

January 28.

Corrections in Nomenclature: Orang Outang; Ca'ing Whale.

KINDLY allow me a line or two in NATURE to point out that *Orang outang* is not the correct designation for the large anthropoid of Borneo and Sumatra, although it has now obtained, perhaps, what may seem a prescriptive right in our language. Nevertheless, it is as well to be accurate as not. *Orang utan* (or *outan*, if preferred), the correct Malay name for this ape, signifies (as is well known) *Orang*, man, and *utan*, forest, i.e. the forest man, in contradistinction to the *Orang dusun*, or village (civilised) man. *Orang utang* (or *outang*) is nonsense. *Utang* means *debt*, something *owing*. The correction has been made often before, but the occurrence of the erroneous combination in the latest abstract of the *Proceedings* of the Zoological Society and in a recent zoological work induces me to venture, in the interest of accuracy and of those who understand the Malay language, again to direct attention to the proper spelling.

In a previous issue of NATURE (March, 1901) you kindly afforded me space to point out the erroneous use also of "ca'ing" for "ca'in," as the Anglicised (or Scotticised) appellation for *Globocephalus melas*. My friend Sir H. H. Johnston, I observe, in his recent elegant work on British mammals uses "ca'ing whale." I hope he will accept this small correction for his second edition. Ca'in is, of course, really equivalent to "call in." "Call" in the Scottish vernacular = ca' = drive: the "drive in" whale. Here the use of "ca'ing" = calling would be inappropriate, as the whale does not "call," either in the sense of "bellow" or "drive." If, however, it be argued that "ca'ing" does stand for "calling," the essential word "in" is omitted, and ought to be supplied. The pilot whale is the species, which in the islands to the north of Scotland so frequently occurs in large "schools," when it is invariably "driven in" for capture on the shore by a surrounding fleet of boats.

HENRY O. FORBES.

Museums, Liverpool, January 30.

Strange Winter Scenes connected with Lough Neagh.

At the close of the long frost in February, 1895, strange phenomena occurred in connection with Lough Neagh, in the north of Ireland, the largest lake in the United Kingdom, and one of the larger ones of Europe, covering as it does an area of upwards of 150 square miles. The lake had been frozen over for a fortnight, and thousands of people had indulged in skating on ice almost as smooth as glass.

On February 22, the last day but one of the skating, though unknown to the multitudes gathered near Antrim, the ice in the central portion of the bay broke up, but left intact a sheet of about a third of a mile wide along the south-eastern shore. At a point about six miles from Antrim, this unbroken shore portion was at intervals of a few yards for a mile and upwards raised into little tunnels or bridges, from beneath which pieces of ice, large and small, along with some boulder stones of considerable size, were shot on to the land, eventually forming a ridge varying in height from two to fourteen feet, and perhaps twenty feet broad at the base. The jingling and crashing heard

during the operation, which lasted for two days, were very great, and to some persons residing near most alarming. Ice has often been seen piled up along the shore at certain points, five or six feet high, but this has been shore ice thrown up by waves, whereas the ridge referred to was not shore ice, that, as stated remaining unbroken for a third of a mile out.

I met with only one person who had witnessed a similar scene to the one described. She had resided near the lake all her life, and remembered the long frost of 1814-15, when the lake was frozen over and a great ice ridge was thrown up. On both occasions a person could walk along the road near the lake and yet not see it, in consequence of the intervening ridge. Where did the ice forming this ridge come from? And what was the force employed to convey it to, and shoot it on to the shore?

At a spot on the same south-eastern shore of Antrim Bay, about midway between that previously mentioned and Antrim, a similar scene in one respect, but on a greatly reduced scale, was witnessed. A man when passing along a lonely, wooded part of the road, at a considerable distance from the lake, heard great hissing and fizzing, in a jerky, intermittent manner. On making his way through the underwood to the place from whence the sounds proceeded, he was astonished to see a large stone, estimated to be several hundredweight, being ejected from beneath the raised ice, and at the same time large quantities of water squirted from the apertures near it. Immediately the propelling force ceased, the stone fell back and the squirting stopped. It was somewhat risky to venture near, but three persons did so to see if they could withstand the propelling force of the water giant, but they found the effort ineffectual, and got drenched for their pains. Through some obstruction, or the stone being too heavy, it was not ejected from the lake.

It would be interesting to know the causes of these phenomena, and also whether they have been observed in connection with lakes elsewhere.

If further information is desired by any reader interested in this matter, I shall be happy to give it if able.

The Manse, Antrim, January 26.

W. S. SMITH.

The α Rays of Radium.

In Mr. Soddy's article on radio-activity in your issue of January 28, he remarks as peculiar the fact that the α rays possess the "property of being more difficult to deviate for any given strength of field the greater the distance of air traversed." Surely if these rays consist of positively charged material particles, their velocity must diminish in proportion to the distance of air traversed, and hence their magnetic effect, and consequently their deviability, must diminish also.

I have unfortunately missed Prof. Rutherford's proof as to the probable difference in speed with which the α particles from the successive disintegrations are shot off. Could Mr. Soddy supply the reference, as there seems no obvious reason why this should be so?

J. T. NANCE.

Bromsgrove School, Worcestershire, February 2.

It is true, as Mr. Nance points out, that the velocity of the α rays may be expected to diminish in proportion to the distance of air traversed, and it follows, therefore, that the magnetic deviability should correspondingly increase, for the displacement experienced by the particle in unit time by a constant magnetic force from the position it would occupy if no force were acting is constant. With diminishing velocity the displacement in unit distance, and therefore the angular deviation, must increase.

The complexity of the α rays of radium was referred to by Prof. Rutherford in his paper in the *Phil. Mag.* for February, 1903, in a footnote to p. 184. Only 25 per cent. of the α rays come from the radium, the remainder originating from its successive disintegration products, viz. the gaseous emanation and the matter causing the excited activity. As these three types of matter have no resemblance whatever in their material nature, it would be a remarkable coincidence if the α particles expelled in their several disintegrations happened to possess the same momentum in each case. This is the condition necessary for the α rays of radium to be deviated as a homogeneous pencil in a magnetic field.

FREDERICK SODDY.